Brain Injury Resulting From Falls Among Elderly Persons

To the Editor: The Research Letter by Dr Kannus and colleagues[1] reports that hospitalization rates for fall-induced severe head injury among Finns aged 80 years and older have increased. This is an important trend that US data cannot yet address. The US National Hospital Discharge Survey includes hospital admissions for traumatic brain injury (TBI), a defined severe head injury, but not underlying causes. Increasing US rates of hip fracture,[2] primarily a fall injury, and of fatal falls in the United States[2] and Finland[3] suggest that fall-induced nonfatal TBI rates in the United States also may be increasing. Morbidity data including TBI cases (with underlying causes) will be available shortly from the recently expanded National Electronic Injury Surveillance System All Injury Program, soon to be released by the US Consumer Product Safety Commission and the Centers for Disease Control and Prevention.

Among elderly persons in the United States, falls are the underlying cause of a large proportion of fatal TBI. We analyzed National Center for Health Statistics Multiple Cause of Death Public Use Data[4] for people aged 80 years and older, using the Guidelines for the Surveillance of Central Nervous System Injury to ascertain fatal TBI cases, and external cause of injury (E) codes to categorize falls. From 1989 to1998, the crude rate of fall-induced TBI deaths increased 59.6% (from 19.3 to 30.8 per 100 000). The proportions of TBI deaths from falls increased 57.8% among men (from 11.6% to 18.3%) and 42.4% among women (from 17.7% to 25.2%).

Kannus et al suggest that these trends may partially result from increased functional impairments among older adults. While this may be true in Finland, US data from the National Health Interview Survey[5] indicate that from 1988 to 1998 the proportion of people aged 65 years and older who reported their health as "fair to poor" declined about 3%.

The reasons for the proportionate increases in fatal TBI due to falls are unclear. Greater use of computed tomography and improved imaging may have increased TBI diagnoses, which would bias the estimates upward. From 1989 to1998, there also may have been changes in reporting fall-related events on death certificates.

Among older adults, changes in demographic characteristics and health behaviors may contribute to increasing rates of fatal falls. Further research is needed to identify the circumstances and mechanisms of falls and other factors that contribute to fall risk. Such data will provide valuable insights into the underlying causes of fall-related TBI among older adults, and help guide the development of effective prevention strategies.

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1. Kannus P, Palvanen M, Niemi S. Time trends in severe head injuries among elderly Finns. *JAMA*. 2001;286:673-674.
2. Stevens JA, Hasbrouck LM, Durant TM, et al. Surveillance for injuries and violence among older adults. *Mor Mortal Wkly Rep CDC Surveill Summ*. 1999;48:27-50.
3. Kannus P, Parkkari J, Koskinen S, et al. Fall-induced injuries and deaths among older adults. *JAMA*. 1999;281:1895-1899.
4. Centers for Disease Control and Prevention, National Center for Health Statistics. *Epidemiology of Traumatic Brain Injury in the United States*. Available at: http://www.cdc.gov/ncipc/dacrrdp/tbi.htm. Accessibility verified October 17, 2001.
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In Reply: Drs Stevens and Adekoya share our view that TBI is one of the most serious consequences of falling and that a large proportion of TBIs among elderly persons are fall related.[1,2] It is unfortunate that currently the US National Hospital Discharge Survey does not include codes for the underlying causes of the TBI, because such data would provide important clues for injury prevention. We are pleased that US morbidity data, including TBI cases with underlying causes, will be available soon, although it will take several years before time trends can be analyzed.

The analysis by Stevens and Adekoya of the data for fatal TBI cases among elderly persons in the US between 1989 and 1998 is of interest, and their observation of the increasing rate of fall-induced TBI deaths is in line with the findings of our Research Letter. The problem is the exact reasons for the increase in the age-adjusted or age-specific incidence (that is, the average in-

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Letters Section Editors: Stephen J. Lurie, MD, PhD, Senior Editor; Jody W. Zylke, MD, Contributing Editor.

dividual risk) of these injuries are unknown. In our letter, we suggested that an increase in the average risk of falling may partly explain the phenomenon (today elderly people may, on average, be functionally less capable than in the past), or some may now sustain more serious falls than their predecessors. On the other hand, Stevens and Adekoya note that from 1988 to 1998 the proportion of people in the United States aged 65 years or older who reported their health as "fair to poor" declined about 3%. Such general survey questions of health may, however, be insufficient in identifying the true secular changes in risk of falling; particularly among those who are at the highest risk.

Finally, we agree with Stevens and Adekoya that effective prevention of fall-induced injuries among elderly people requires accurate knowledge of the circumstances and mechanisms of falls and other factors that contribute to fall risk. Concerning the mechanisms of falls, we have recently reported the results of 2 prospective controlled studies that describe the detailed injury mechanisms of hip fractures[3] and upper extremity fractures[4] among elderly people.

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1. Kannus P, Parkkari J, Koskinen S, et al. Fall-induced injuries and deaths among older adults. *JAMA*. 1999;281:1895-1899.
2. Kannus P, Palvanen M, Niemi S, et al. Increasing number and incidence of fall-induced severe head injuries in older adults. *Am J Epidemiol*. 1999;149:143-150.
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4. Palvanen M, Kannus P, Parkkari J, et al. The injury mechanisms of osteoporotic upper extremity fractures among older adults: a controlled study of 287 consecutive patients and their 108 controls. *Osteoporos Int*. 2000;11:822-831.

Graduating Residents' Perceptions of Their Preparedness for Practice

To the Editor: Dr Blumenthal and colleagues[1] found that graduating US medical residents felt unprepared for several aspects of medical practice. However, Blumenthal et al failed to ask about residents' preparedness for several additional important aspects of current medical practice.

For instance, do residents have any competence in coding their work? Do they understand the differences between ICD and CPT systems? Do they have knowledge of the coding standards, especially for evaluation and management work? Do they understand physician work and how its definition changes how they practice medicine?

Do they understand governmental responsibilities related to practice of medicine? Do they understand the OIG, OSHA, the upcoming role of HIPAA? Do they understand their legal responsibilities under law, or where to find out about applicable regulations, or how laws affect their office environments?

Do they understand contracts? They will practice in places where they personally will sign contracts, or contracts will affect relationships to insurers or their workplace environment. Do they have an understanding of their rights and how to protect them?

Do they understand medical finance and how an office runs? They need to know about resource costs in their practices, how to measure productivity, any how to properly handle money. What about investing, pensions, benefits?

Do they understand the history of modern medicine in the United States or in whatever country they intend to practice? How did medical care develop its patterns? What are the broader social problems they will face in the next decade in providing care?

I have offered to teach these matters to residents in several programs over the last 19 years, and there has been virtually no interest from residents, colleagues, or program directors. In my view, failure to address the subjects above makes for an ill-prepared resident in the context of what I know to be the demands of modern medical practice.

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1. Blumenthal D, Gokhale M, Campbell EG, Weissman JS. Preparedness for clinical practice: reports of graduating residents at academic health centers. *JAMA*. 2001;286:1027-1034.

To the Editor: Dr Blumenthal and colleagues[1] found that most graduating residents felt prepared to diagnose and treat low back pain. This is an intriguing finding, given that many rheumatology programs provide inadequate training for such tasks. Furthermore, rheumatology education in adult primary care residency programs is generally limited to less than 1 month in 3 years, despite the fact that these problems represent 20% to 40% of presenting complaints among patients in primary care.[2]

It thus seems reasonable to question the use of resident perspective as a measure of preparedness for clinical practice. While faculty observation of clinical evaluation seems an appropriate technique to provide objective assessment, who validates the faculty?

Assessing preparedness for clinical practice is not a matter of asking residents for their perspectives. It is a matter of ensuring that their patients receive appropriate care by physicians whose skills have been objectively assessed, and who know their limitations.

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1. Blumenthal D, Gokhale M, Campbell EG, Weissman JS. Preparedness for clinical practice: reports of graduating residents at academic health centers. *JAMA*. 2001;286:1027-1034.
2. Rothschild BM. Primary care rheumatology. *Arch Intern Med*. 1982;142:26-27.
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To the Editor: Dr Blumenthal and colleagues[1] report that 32% of graduating anesthesiology residents felt unprepared to man-

age chronic pain. The authors used this finding to support their conclusion "that gaps may still exist in the preparedness of physicians to manage the full range of patients . . . they may confront as practitioners." As anesthesiologists and educators, we see this response as appropriate and as confirmation of exactly the mode of education we now use to teach anesthesiology residents.

Anesthesiology residency is not intended to train physicians to treat chronic pain. There is neither time, nor is there a need in view of the nature of the daily practice of most consultant anesthesiologists. Anesthesiology residents are taught to care for patients in acute pain, primarily those who have sustained trauma or will undergo an operative procedure. The Accreditation Council for Graduate Medical Education requires only 1 month of pain management during anesthesiology residency.[2] Consultants in anesthesiology must be conversant with strategies for acute pain relief. Indeed, 93% of anesthesiology residents in the survey of Blumenthal et al reported that they were prepared to treat acute pain.[1] However, anesthesiology residents typically get little more than an introduction to chronic pain during their training. Formal fellowships in pain management have been available for nearly a decade, and the American Board of Anesthesiology has established a formal examination process leading to the Certificate of Added Qualifications in Pain Management.

We applaud the 32% of anesthesiology residents who described themselves as unprepared to manage chronic pain. If there is any shortcoming in anesthesiology training programs, perhaps it is revealed by the 68% of anesthesiology residents who reported they were prepared to treat chronic pain. We would have hoped that they had gained enough exposure to the pain medicine subspecialty to realize the large amount they would yet have to learn before they could consider themselves "prepared" to treat chronic pain.

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1. Blumenthal D, Gokhale M, Campbell EG, Weissman JS. Preparedness for clinical practice: reports of graduating residents at academic health centers. *JAMA*. 2001;286:1027-1034.
2. Accreditation Council for Graduate Medical Education. *Anesthesiology Training Program Requirements*. Chicago, Ill: Accreditation Council for Graduate Medical Education; January 2001.

In Reply: We agree with Dr Kaufman that residents should ideally be prepared to manage all the professional issues they are likely to encounter, including the organizational, policy-related and business aspects of practice in our tumultuous health care system. Our survey did, in fact, inquire about residents' preparedness to participate in certain of these activities. Because of inherent limitations to the length of surveys, we could not cover these matters in the detail we would have liked. Some of our findings concerning these skills are reported elsewhere.[1] Deficits in these nonclinical skills are often substantial. For example, we found that 40% of third-year internal medicine residents felt unprepared to participate in quality assurance activities.

Dr Rothschild argues that residents' perception of their preparedness to handle clinical practice is an imperfect measure of their true competence. We certainly agree that it would be preferable to have better measures than perceived preparedness for assessing clinical competence at every stage in physicians' practice lives. Accrediting organizations and boards are actively assessing such techniques, and their application, while desirable, will prove costly, controversial, and complicated. In the meantime, we feel that assessing self-perceived preparedness provides valuable, affordable, national perspective on the training experience. If, as Rothschild suggests, physicians tend to overestimate their competence, then the areas where residents themselves feel less than fully prepared may deserve special attention from training programs.

Dr Merrill and colleagues highlight one of the dangers in undertaking comprehensive studies that attempt to collect clinically meaningful, comparable data on a range of medical specialties. Our measures of preparedness were informed by focus groups of residents from each surveyed specialty in Boston training programs and by sharing our instruments with training directors of Boston residencies. Nevertheless, some of our indicators may not have adequately characterized the intended curricula of all the specialties we surveyed. This seems to have been the case with chronic pain management in anesthesia. Merrill et al suggest that few, if any, residents should feel prepared to manage chronic pain at the end of their residencies. If that is the case, perhaps more work needs to be done in providing anesthesia residents some perspective on what they do not know, given the plurality in our study who felt they were somewhat or very prepared.

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1. Weissman JS, Campbell EG, Blumenthal D. How does market competition affect resident physicians' views toward managed care? *Am J Med*. 2000;109:437-442.

Physical Activity Counseling in Primary Care

To the Editor: The Activity Counseling Trial Research Group[1] reported that among subjects randomized to receive advice about exercise, the proportion of patients "meeting physical activity goals" (defined as moderate or vigorous activity 5 or more days a week) between baseline and 24 months increased from 1.5% to 16.4% among men and from 0.8% to 14.3% among women, representing relative increases of 993% (men) and 1688% (women).We believe that the authors should have placed much greater emphasis on changes from study subjects' baseline condition. The authors' presentation makes the intervention appear fairly

ineffective, particularly in men, while an examination of the change from baseline makes the results appear highly effective.

For instance, according to the physical activity objectives in Healthy People 2000,[2] Powell and Blair[3] estimated the deaths from coronary heart disease, colon cancer, and diabetes mellitus that could be avoided by a change from 22% to 30% in the proportion of the US population engaging in regular or vigorous physical activity, a relative increase of 36%. In the activity counseling study, regular physical activity was defined as light to moderate activity 5 or more days a week, and vigorous physical activity was defined as vigorous activity 3 or more days a week. These relative changes reported by the Activity Counseling Research Group are approximately 30-fold greater than those modeled by Powell and Blair.

In 1995, the average age at death was 77.1 years for coronary heart disease, 73.3 years for colon cancer, and 72.0 years for diabetes, while the average number of years of life remaining for persons surviving to these ages was 9.9, 12.1, and 12.9, respectively.[4] We estimate that if the Healthy People 2000 objectives had been met, the number of deaths caused by these 3 diseases and that could have been avoided in 1995 were 26 950 for coronary heart disease, 2248 for colon cancer, and 2548 for diabetes. Thus, if the proportion of the US population engaging in regular or vigorous activity increased from 22% to 30%, our analyses suggest that each year, the nation might avoid the loss of 326 895 years of potential life. According to the results of the Activity Counseling Trial, such changes in the physical activity level of the US population are realistic and would result in huge public health benefits.

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Disclaimer: The views expressed in this letter are those of the authors only and should not be interpreted as the official position of the US Air Force or the Department of Defense.

1. Activity Counseling Trial Research Group. Effects of physical activity counseling in primary care: the Activity Counseling Trial: a randomized controlled trial. *JAMA*. 2001;286:677-687.
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4. *Vital Statistics of the United States, 1995*. Hyattsville, Md: National Center for Health Statistics; 1998.

This letter was shown to Dr Simons-Morton, who declined to reply.—ED.

Meningococcal Disease in College Students

To the Editor: In their case-control study of risk factors for meningococcal disease in college students, Dr Bruce and colleagues[1] report that 4 of the case subjects (8%) died before exposure histories could be obtained, so proxy patients were interviewed. The authors did not exclude the 4 deceased case subjects, nor did they identify and interview proxies of the 4 matched controls. It is possible that this introduced a small amount of bias because of the differential misclassification of exposure. The case subjects were more likely than the control subjects to have had their exposures misclassified. The odds ratios (ORs) may have been biased toward or away from the null value.[2] For example, the lack of a significant association between active smoking and risk of disease may have been caused by underreporting by the 4 proxy patients.

Since the case-fatality rate of meningococcal disease in adolescents and young adults may be as high as 22%,[3] future related case-control studies should include control proxies if case proxies are used. This practice is known as symmetric data collection and has been used in case-control studies in which the case subjects were demented.[4,5] Symmetric data collection would lead to nondifferential misclassification of exposure, which would bias the ORs toward the null.[2] In this situation, case subjects would be just as likely as control subjects to have their exposures misclassified. This attenuation of the OR then could be counteracted by increasing the study sample size. There are other study design alternatives available to the investigator when some or all of the case subjects cannot supply exposure data; however, the study design should not be structured so that proxy respondents are used for some of the cases but none of the controls.[5]

Zuber D. Mulla, MSPH, PhD
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1. Bruce MG, Rosenstein NE, Capparella JM, Shutt KA, Perkins BA, Collins M. Risk factors for meningococcal disease in college students. *JAMA*. 2001;286:688-693.
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3. Harrison LH, Pass MA, Mendelsohn AB, et al. Invasive meningococcal disease in adolescents and young adults. *JAMA*. 2001;286:694-699.
4. Amaducci LA, Fratiglioni L, Rocca WA, et al. Risk factors for clinically diagnosed Alzheimer's disease: a case-control study of an Italian population. *Neurology*. 1986;36:922-931.
5. Nelson LM, Longstreth WT Jr, Koepsell TD, van Belle G. Proxy respondents in epidemiologic research. *Epidemiol Rev*. 1990;12:71-86.

In Reply: We performed univariate and multivariable analyses of the case-control data, which included all 50 case subjects and 148 control subjects; however, we also performed a subanalysis in which the 4 dead case subjects and their associated 12 control subjects were excluded. Results from the initial univariate and multivariable analyses remained unchanged in the subanalysis. We agree with Dr Mulla that to reduce differential (nonrandom) misclassification bias, researchers conducting case-control studies should consider the use of control proxies, particularly when a substantial number of case subjects are either mentally incapacitated or have died; however, in this case using matched student control subjects, not control proxies, did not result in any substantial differential misclassification bias.

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Using Meta-analysis to Answer Clinical Questions

To the Editor: In their meta-analysis, Dr Ioannidis and colleagues[1] found that randomized and nonrandomized studies often reach similar conclusions, although nonrandomized studies may yield larger estimates of the effect size. I agree with the authors that nonrandomized evidence can help explore clinical questions. However, analyzing sources of heterogeneity among randomized and nonrandomized trials might be more informative than simply pooling treatment results from only randomized clinical trials. Especially in the case of an unclear treatment effect, all available information should be used to resolve the uncertainty in outcome between studies. Important information from nonrandomized clinical trials should not be ignored; instead, heterogeneity in design, data collection, and analysis should be explored. Even in randomized clinical trials, the method of randomization might lead to differences in outcomes.

Identifying all relevant data to explore differences in methodological aspects of studies is important not only in evaluating the efficacy of therapeutic and preventive interventions but also in estimating the accuracy of diagnostic tests.[2,3]

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1. Ioannidis JPA, Haidich A, Pappa M, et al. Comparison of evidence of treatment effects in randomized and nonrandomized studies. *JAMA*. 2001;286:821-830.
2. Laheij RJ, Straatman H, Jansen JB, Verbeek ALM. Evaluation of commercially available *Helicobacter pylori* serology kits: a review. *J Clin Microbiol*. 1998;36:2803-2809.
3. Lijmer JG, Mol BW, Heisterkamp S, et al. Empirical evidence of design-related bias in studies of diagnostic tests. *JAMA*. 1999;282:1061-1066.

To the Editor: Dr Ioannidis and colleagues performed a meta-analysis of previously published meta-analyses to address the results of randomized vs nonrandomized studies.[1] For clinicians who are not familiar with this method, a number of questions are likely to arise.

First, do meta-analyses yield reliable results? If so, this implies that biases of various studies cancel each other out through meta-analysis. Is this assumption generally accepted? Second, how does meta-analysis avoid compounding previous errors, especially in an environment in which peer influence is considerable? It appears that a meta-analysis may serve only to make scientists aware of their colleagues' research and may not substitute for original work. In this case, what does a meta-analysis of meta-analyses stand for? The meta-analyses presented by the authors originate from a small spectrum of medical specialties and institutions. They are certainly neither random in their inception nor cross-sectional in their representation. Is it possible that meta-analyses ad infinitum only perpetuate or even amplify faults of the original studies? Finally, was all this analysis really worth the effort just to reach the conclusion that there are discrepancies between randomized and nonrandomized studies?

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1. Ioannidis JPA, Haidich A, Pappa M, et al. Comparison of evidence of treatment effects in randomized and nonrandomized studies. *JAMA*. 2001;286:821-830.

In Reply: We agree with Dr Laheij that exploration of heterogeneity sources may provide valuable information, especially for controversial medical topics for which therapeutic or preventive effects of different magnitude are obtained by different study designs.[1] We also agree that empirical meta-analytic evaluations can be applied in other types of studies as well.

Dr Theodoropoulos and colleagues reduce meta-analysis to a method that simply lumps information and biases. We do not share this view. Meta-analysis has the potential to systematically examine the strengths and weaknesses of the accumulated evidence, explore heterogeneity between studies (including those of different designs), and identify potential biases. As a form of original research, meta-analysis does not compete with or substitute for other original work. It functions at a different level than single original studies and has been widely accepted as the highest level in the hierarchy of evidence.[2,3] We welcome criticism of meta-analysis, which may help refine further its methods. However, when a meta-analysis shows that no more research is needed or that there are overt biases that should be appropriately corrected, then it is equivalent to misconduct to perform further clinical studies or to conduct new studies without correcting known biases.[4]

Finally, the 45 medical topics that we studied represent a necessarily limited sample, suggesting that there is ample room for further similar empirical evaluations that scrutinize several meta-analyses across diverse fields.[5] Since these research designs require extensive effort, we think that they should be performed to answer questions of critical interest in the biomedical sciences and elucidate the sources of diversity among the constituent studies.[6] In this context, we would dare to speculate that understanding the discrepancies between the 2 major forms of clinical research is probably worth the effort.

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1. Lau J, Ioannidis JP, Schmid CH. Summing up evidence: one answer is not always enough. *Lancet*. 1998;351:123-127.

2. *Acute Pain Management: Operative or Medical Procedures and Trauma*. Rockville, Md: Agency for Health Care Policy and Research, US Dept of Health and Human Services; 1993. AHCPR publication 92-0023.
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5. Ioannidis JP, Haidich A-B, Lau J. Any casualties in the clash between randomized and observational evidence? *BMJ*. 2001;322:879-880.
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Politics and Medicare

To the Editor: In his review of the second edition of Theodore Marmor's classic book *The Politics of Medicare*, Dr Kane[1] allows that "[t]his book provides just what its title promises." Who could object? Surprisingly, Kane does, arguing that "for most readers the politics is less interesting than the substance." Serious books, especially influential texts, should be subject to rigorous critical review. But Kane's assertions about Medicare's politics and history, and about the politics of health policy more generally, are doubly misplaced. Besides being only vaguely related to Marmor's book, they are thoroughly contradicted by the growing body of work in the field of health policy and politics.

Kane's main claims—that politics is of little interest to "most readers" and that a program's substantive attributes can be readily separated from its political context—are factually wrong and analytically hazardous. Those who study the politics of health policy care deeply about the substance of Medicare policy and its consequences for providers, beneficiaries, and the nation as a whole. But Medicare is in fact a prime example of just how inseparable substance is from politics. Few would dispute that the failure to enact policy changes—in Medicare as in other areas—is a product of politics and warrants close scrutiny by political analysts. The substance of policies that are enacted is also shaped profoundly by politics and, in turn, shapes politics. Marmor's *The Politics of Medicare* recognizes this fundamental point and seeks to illuminate how the character of the program has followed so directly from its politics.

Kane writes that *The Politics of Medicare* is "neither an historical text nor a policy analysis" and "does not offer a coherent account of the evolution of Medicare or an analysis of the effects of these changes. It was not intended to." Kane has every right to disagree with Marmor's argument, but there is no question that *The Politics of Medicare* is meant to be a coherent account of Medicare's evolution.

Moreover, even if Kane were correct about Marmor's motives, criticizing a book on the grounds that it was not intended to be something it is not is a strange strategy. Kane fails altogether to discuss or critique the contributions that Marmor's book does make and that have earned it a permanent place in the literature of health care policy. Kane notes, for instance, that the second edition incorporates the original text of Marmor's 1973 first edition, widely considered the definitive account of the origins of Medicare.[2] Yet he makes no mention of the exportable conceptual framework that Marmor used, which many scholars have drawn on to identify and dissect the political predicates of other health care policy decisions in the United States and other countries.[3] And Kane scarcely addresses or assesses the substance of the book's rendering of the institutional structures, political forces, actors, and machinations that led to the program's creation and current form. In short, he fails to relate much of the basic information about methodology and content that readers should expect from an expert review.

Instead, Kane devotes much of his review to describing the book that he clearly wishes Marmor had written, one focusing largely on technical issues that Medicare faces. In the process, he repeats a mistake that too often characterizes discussions of Medicare and, more broadly, of health care policy: equating substance with technical fixes and assuming that deep political divisions can thus be avoided. The seemingly endless quest for apolitical, technocratic solutions distracts attention from recognizing the political constraints that condition even the best-laid policy plans and from focusing on the need and possibilities for more fundamental shifts in policy.

Kane makes it clear that he is unhappy with the state of Medicare. He writes, "It remains an insurance program; it is not even a managed care program." But what will determine whether Congress changes Medicare along the lines that Kane would prefer? The answer is the politics of Medicare—to which there is no better guide than Marmor's book.

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1. Kane RL, reviewer. *JAMA*. 2001;285:1220. Review of: Marmor, TR. *The Politics of Medicare*.
2. Review symposium on *The Politics of Medicare*: a thirty-year retrospective on Ted Marmor's classic work. *J Health Polit Policy Law*. 2000;26:119-174.
3. Touhy CH. The politics of Medicare north and south. *J Health Polit Policy Law*. 2001;26:161-167.

In Reply: Theodore Marmor has a strong group of supporters who have risen eloquently to his defense, and indeed to that of the field of political science. First, Marmor needs no defense. His original book on Medicare is indeed a classic. The book I reviewed was the second edition. The review was requested because the

book offers relevant new material. I read it in the context of what is happening and tried to put it into that context for practitioners. Although I did not mean to disparage the field of political science, I did want to point out the limitations of the book.

Prompted by the letter from Mr Frankford and colleagues, I reviewed my review. I believe I made 2 substantial errors. First, I failed to acknowledge the achieved and deserved status of the original volume. Second, I suggested that the book did not provide historical insights. Indeed, it does offer great insights into just how Medicare was originally legislated.

Some view politics as the art of the possible, but it also seems worthwhile to look at the potential of a program to understand why it has not achieved that goal. The politics of Medicare have changed while they have remained the same. The stakeholders are more powerful and the stakes are larger. States and other units of government, in addition to the various providers of care, have much to gain or lose. The people who are ultimately affected, the older consumers of care, have put their oar in the water more deeply, but it is unclear just how much pull they have or in what direction they are pulling.

The areas of concern have shifted over time. Issues of access have yielded to concerns about cost. Like most public programs, Medicare has been asked to do more for less. Prospective payment has not proved the panacea some had hoped. Issues of fairness with regard to payment and coverage abound. As a major player in the health insurance game, Medicare is affected by the changing environment. Customer focus and error reduction vie for attention with expanded benefits and solvency.

Any longings for a book not written were simply my efforts to identify issues and areas that I believed deserve to be addressed at this point in Medicare's history. The volume obviously has many long suits, despite what I felt were its shortcomings. Satisfaction is predicated on expectations; perhaps mine were too high.

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RESEARCH LETTER

Toxic Landscaping of Facilities for Developmentally Disabled Adults

To the Editor: Developmentally disabled adults are at a high risk for unintentional poisoning. Plant foliage surrounding residential facilities and day-training centers serving these adults may pose a significant potential poisoning hazard. During a 4-week period in August to September 2000, 4 unintentional plant poisonings of developmentally disabled adults, including 1 fatality, were reported to the Illinois Poison Center by such residential facilities.

Report of Cases. A 45-year-old developmentally disabled man was found unresponsive 1 hour after he was seen eating twigs and plants. Prehospital telemetry demonstrated asystole, and he was pronounced dead in the emergency department. Postmortem examination revealed 300 mL of mulch, grass, twigs, and pine needles resembling those of the yew plant (*Taxus* spp) in his stomach, and no other apparent cause of death. Toxicologic analysis of the bile and blood confirmed the presence of alkaloids from the yew plant.

During this time, 3 other developmentally disabled adults, aged 20 to 50 years, were also admitted to emergency departments for plant ingestion. Two patients ingested leaves and berries from yew, while the third consumed honeysuckle berries. All were observed for 24 hours, and none had adverse effects.

Comment. Although plant ingestions represent the fourth most common call to poison centers, most ingestions are not associated with significant morbidity or mortality.[1-3] However, 3 of these 4 cases involved the yew plant, a popular evergreen shrub containing toxic alkaloids that may cause sudden and potentially fatal cardiac toxicity.[4,5] Developmentally disabled adults may ingest large amounts of plants and other potentially toxic substances. Based on these 4 cases, we suggest identification and removal of all toxic plants from areas surrounding facilities for developmentally disabled persons.

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